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ABSTRACT

The author aims: (1) to show that generative phonology uses essentially the method of internal reconstruction which has previously been employed only in diachronic studies in setting up synchronic underlying phonological representations; (2) to show why synchronic analysis should add the comparative method to its arsenal, together with whatever adjustments in theory are required to make this accommodation; and (3) to present some empirical results of a first approximation to a pandialectal phonology which has been worked out by the author. This paper represents a revision of a paper read at the Conference on Historical Linguistics in the Perspective of Transformational Theory held at the University of California, February 1, 1969. A shorter and more revised version will appear in the Conference papers to be published by the Indiana University Press. (AMM)

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THE INTEGRATION OF LINGUISTIC THEORY: INTERNAL RECONSTRUCTION AND THE COMPARATIVE METHOD IN DESCRIPTIVE LINGUISTICS

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A much shorter and considerably revised version will appear in the volume of papers read at the Conference just mentioned to be published by the Indiana University Press. Since a number of papers at the Conference (including the present paper) represent considerable departures from the (homogenist) assumptions of orthodox transformational theory, the volume will probably substitute "Generative" for "Transformational" in its title.

THE INTEGRATION OF LINGUISTIC THEORY: INTERNAL RECONSTRUCTION AND THE COMPARATIVE METHOD IN DESCRIPTIVE LINGUISTICS

(i)

A decade ago Stockwell (1959) proposed for linguistic analysis pandialectal segments constructed by matching up, through phonetic skewing, the contrasts found in individual dialects "in such a way as to retain only the minimum inventory, the over-all pattern." Although linguistic theory current at that time prevented the general acceptance of this idea, I hope to show that it should be given a new lease on life in terms of a more dynamic model of language than the static one that we have all inherited from de Saussure. I am not proposing that we abandon the current transformational model, but that we adapt it to a new point of view in which the emphasis is on communicating, rather than simply on the production of utterances. This will entail admitting a good deal more data than have been previously admitted, since the child acquiring his native language will be understood to be formulating a grammar that will accommodate much more of the system of his native language than the subset of it he uses in what de Saussure termed execution. Such a model will require a more subtle means of organizing the disparate data than the simpler idiolectal model.

In this first section I wish briefly to show that generative phonology uses essentially the method of internal

reconstruction, which has previously been employed only in diachronic studies, in setting up synchronic underlying phonological representations. Along the way, I hope to give some indications of why phonology is the crucial area for assessing the theory being espoused in this paper. In the second section I wish to show why synchronic analysis should add the comparative method to its arsenal, together with whatever adjustments in theory are required to make this accommodation. In the third section, I wish briefly to present some empirical results of a first approximation to a pandialectal phonology which has been worked out by me.

The most interesting and significant examples of the use of the method of internal reconstruction in synchronic analysis are found in Chomsky and Halle's (1968) The Sound Pattern of English (hereafter SPE). For they have added a good deal of theory to the method and provided notational ways of formulating their theory. The most interesting examples of what is essentially the method of internal reconstruction are the justification of an underlying voiceless velar fricative in righteous (SPE 233), which accounts for the different behavior of the root vowel and //t// in this word and in ignition, and the justification of geminate consonants and an underlying final //e// in giraffe (SPE 148-50).

What is important to stress here is that differences in data do not create differences in method or theory. The aim of synchronic analysis requires the exclusion of data not available to a child but available to a historical linguist

and therefore admissible in diachronic analysis. The historical linguist has a differnt aim from that of the synchronic linguist, requiring that his reconstructions square with earlier reconstructions as well as with the attested data. Where the child acquiring a dialect in which taut, tort, and taught are homophonous might hesitate between a choice of underlying representations if he had never heard an "r-ful" dialect, the historical linguist has records which decide the issue for him. But this difference in the data that are admitted in the two kinds of analysis no more entail methodological and theoretical differences than do different kinds of synchronic data--e.g. the data of Japanese, Samoan, and Arabic. The results differ because the aims and therefore the data are different.

(ii)

Historical linguists should be pleased to find that they can make a contribution to synchronic analysis and to the unification of linguistic theory. I wish to propose here that it is not too early to begin abandoning the simplifying assumption of SPE (331) concerning the instantaneous acquisition of language by children. I wish to claim that we need what is essentially the comparative method to give an adequate account of the child's prowess in understanding, and even imitating, some of his grandparents, his parents, possibly a servant of a different social grouping, neighbors of different regional idioms, schoolmates of different ethnic or other social backgrounds, and the announcers on the radio and tele-

vision. Not many linguists will wish to question the assertion that the knowledge of his language used by a child in speaking is but a subset of the total knowledge of his language which he uses in communicating with others.

That a single underlying representation and a single set of grammatical rules, constructed by means of the comparative method and indexed by different speakers for application or non-application in their speech production, form the basis of interdialectal communication is a view that is not in accord with current theory, where a single idiom is the basis of all analyses. One of the clearest statements of this point of view is found in Becker (1968:7). This point of view represents the faithful adherence of transformationalists, no less than of structuralists, to the now traditional mode of resolving the Saussurian contradiction between the relatability of contemporaneous dialects of the same language and the claimed unrelatability of prior and posterior states of the same language: "A diachronic fact is an independent event; the particular synchronic consequences that may stem from it are wholly unrelated to it" (de Saussure 1959:84 = 1959:121; cf. 1959:83,91 = 1962:119-20,129).

The position being taken here is that, instead of resolving the Saussurian contradiction by treating related dialects as unrelatable, linguists should have followed the contrary course of relating all dialects, whether diachronic or synchronic, that can be reduced to one system, as the term is conceived of below. Since it is one human in which both

diachronic changes and synchronic differences recur, there is no evident reason for not beginning with the working hypothesis that the acknowledged systematicity of language will be parallel in the two aspects of language. The hypothesis can be corrected when any errors in it are demonstrated. Recent work by Labov and others at Columbia University has amply demonstrated the validity of de Saussure's (1959:14 = 1962:30) characterization of langue: "In separating language [langue] from speaking [parole] we are at the same time separating: (1) what is social from what is individual; (2) what is essential from what is accessory and more or less accidental." Children do in fact relate their grandparents' idioms, as well as the idioms of their contemporaries, to their own. To account for the polydialectal competence of the speaker-hearer, four possibilities (some of which have been made more evident to me through discussion with Gary Parker) are open:

1) There is no system in the child's knowledge of how he communicates with others, but only a congeries of rules of thumb which he memorizes in order to facilitate communication. This contradicts all that is known about the systematic nature of language and the evidence which has been gathered by Labov (1966) in favor of the view that the apparent randomness of certain kinds of idiolectal variation can be subsumed under a larger, communal system. A given speaker will have different outputs in similar linguistic environments varying in such a way as to reflect a system discoverable in recognition tests and in statistical studies of speaking performance in the mass.

2) The child simply ignores differences and makes guesses on the basis of the sheer similarities between his own speech and the speech that he hears. This proposed explanation should be testable. It would seem to entail the inability of the child to imitate the different kinds of speech he is acquainted with and the inability of a person who does not distinguish which from witch or pen from pin to know which possibility is intended by a speaker who, he knows, does make the differences. Since two idiolects are equidifferent, the proposed explanation should predict that they should be intelligible to each other in the same degree. As is well-known, this is not correct.

3) The child's over-all knowledge of the language consists of a plurality of partial, though internally consistent, grammars. How some speakers could organize as many different grammars as would be needed is a problem that boggles the imagination.

4) The child constantly revises a single internalized grammar of English or some other language in such a way as to accommodate all the data of the appropriate sort which he encounters during a decade or more or language acquisition. In considering this hypothesis, it is necessary to remember that dialects, at least the standard ones, differ least in syntax and most in phonology. Some syntactic differences, like the insertion or omission of it in "I hate it that she is leaving," are really lexical. The main problem with the proposed explanation now under discussion involves questions

of how the brain could organize the diversity and fluctuation that would have to be accommodated in a unifired grammar of the sort being postulated.

Three ways of doing this are thinkable, and even plausible. First, it is easy to recognize a more general form of a rule from a less general form already known, for rules become more general by deleting features present in less general statements. It will be rather more difficult to recognize a less general form from a known more general form of a rule, since the less general formulation will have features and values of features present which are quite unpredictable from the more general formulations. To illustrate generalization with a very simple example, the Northerner who neutralizes the accented vowels of Mary and merry will recognize the generalization before both liquids heard in a Bostonian's pronunciation of sailing like selling. Similarly, the Bostonian who drops underlying //r// in harp will presumably recognize what is going on when he hears the other liquid dropped in some Southern States pronunciations of help.

The brain can also predict the unmarked order of critically ordered rules from a marked order in which not all of them apply. The Southerner who first syllabifies the nasal in paten and then changes /r̩/ to [ə] when it is not followed by a vowel will not have to learn the other sequence when he he hears the Britisher or New Englander pronounce pattern like paten, since he will already have been able to predict the unmarked order from his own marked order.[1] He may even have

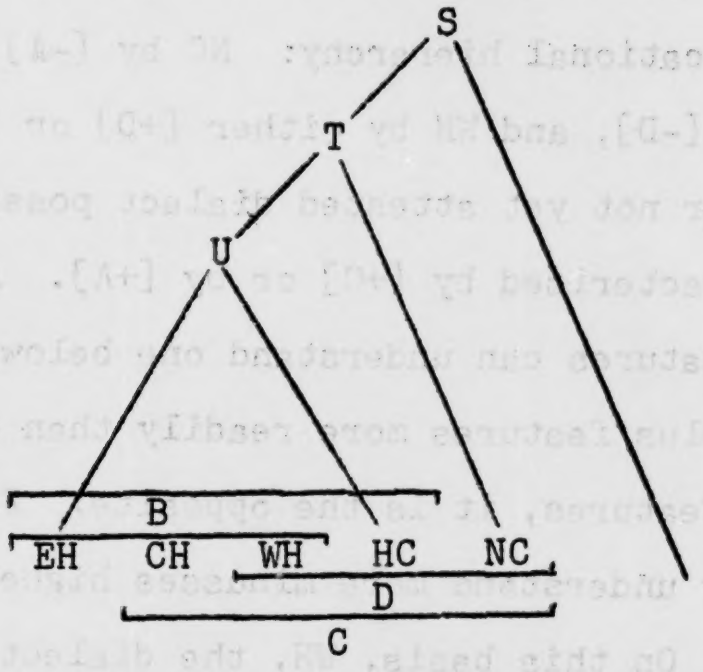
a lexical exception (e.g. modern) or two that have the unmarked order of the relevant rules in his own dialect.

It is clear that a pandialectal grammar would have rules in their least general form and in their marked order, since the more general forms of rules and the unmarked order could be predicted from the other. The rules (or forms of rules) employed by a given speaker in the production of his speech would have to be indexed in some manner.

I wish to claim that a third important means of organizing the materials of polydialectal or pandialectal grammars is with an implicational hierarchy in which most rule applications andnon-applications imply the applications or non-applications of other rules farther down the implicational scale of the language system. Some implications are, of course, trivial; e.g. you cannot change /ɑə/ to [ɑ:] in car until you have first changed //r// to /ə/ (and the underlying //a//, detectable in carriage, to /ɑ/). Others are intradialectal. Thus a Southerner does not have [æɪ] in bad unless he has it also in fast and camp, and not in these unless also in bag, bang, and bash. An implicational scale of this sort seems to me to be much nearer the concept of competence than the statistical formulas based on performance which Labov (1968) is now working with, and to which some of his rules are probably reducible. It may seem at first sight trivial to assert that the deletion of the copula implies its prior contraction, since you cannot contract what has been deleted. But there is no necessary reason why deletion has to imply contraction. Moreover, speakers who

delete the copula in rapid speech may well not delete it in the same style in which they do not contract.

I wish to go beyond these illustrations and show how the whole grammar of a language may be organized with an implicational scale that characterizes the different idioms in a manner first made explicit by DeCamp (1968). The whole idea which I am proposing can be neatly illustrated with an example from Quechua. Below is a schematic version of the interrelationships of some dialects described by Parker (1969:77). The dialects are EH, CH, WH, HC, and NC. A, B, C, and D are individual sound changes, while S, T, and U are bundles of such features.



The complexity, completeness, and limited scope of this problem make it ideal for exemplifying the points that I wish to establish.

The information in the diagram just shown can be reduced to the following implicational scale, formulated with the current conventions:

$$-\begin{Bmatrix} U,A \\ C \end{Bmatrix}_{1\ 1} \rightarrow -\begin{Bmatrix} B \\ D \end{Bmatrix}_{1\ 1} \rightarrow +\begin{Bmatrix} D \\ B \end{Bmatrix}_{1\ 1} \rightarrow +\begin{Bmatrix} C \\ A,U \end{Bmatrix}_{1\ 1} \rightarrow +T \dashrightarrow +S$$

This formula can be further reduced with symbols for rule sets. The broken-line arrow shaft on the right indicates that [S] lies outside the linguistic (polydialectal) system under analysis.

Among the many interesting points in this analysis is the fact that the presence and absence of a rule feature (e.g. [A] or [C]) can be separated on the implicational scale by intervening rule features. Much more important is the fact that individual dialects can be characterized by single features in the implicational hierarchy: NC by [-A], EH by [-C], HC by [-B], CH by [-D], and WH by either [+D] or [+B]. One can even predict other not yet attested dialect possibilities; e.g. a dialect characterized by [+C] or by [+A]. A dialect having more plus features can understand one below it on the scale with fewer plus features more readily than conversely. But with minus features, it is the opposite: Fewer minusses can more readily understand more minusses higher on the scale than conversely. On this basis, WH, the dialect with all the rule features present, should be in a position to understand all other dialects more readily than they could understand it. The unattested dialect characterized by [+A] at the lower end of the scale and NC, characterized by [-A] at the other end of the scale, could be mutually unintelligible, as often happens in such spectrums. In view of this, the criterion of

mutual intelligibility cannot be of much value in dialect studies. One would like to know from psycholinguists whether the predictions concerning degrees of intelligibility made above are in fact empirically justified. One would like to know whether dialects having rules in the unmarked order are more easily understood by dialects having the same rules in a marked order than conversely. One would also like to know whether natural languages allow a rule to move into the unmarked position relatively to one rule when this move puts it into a marked order relatively to another rule with which its relation was previously the unmarked order. Finally, one would like to know whether the weighting of isoglosses depends on the position of a rule feature in an implicational hierarchy more than on the depth of a rule in the over-all ordering. It seems unlikely that it would depend on such a fluctuating performance factor as frequency of occurrence, as some dialectologists have assumed but never systematically tested and verified.

If the mode of organizing dialectal differences which has just been illustrated is general y valid, it is possible to give up the notion of dialect and substitute for it the idea of what may be termed a climacolect, i.e. an idiom characterized by its position on an implicational scale. The idea is not new:

> . . . considering the multitude of isoglosses crossing one another in all possible directions, territorial dialects have often proved to be fictitious entities, established on the basis of arbitrarily chosen features, or on the basis of extralinguistic criteria [like migration routes]. This fact led either to the negation of the very existence of territorial dialects or to the opinion that this

> concept has a relative character, and that the central concept of dialectology should be that of the isogloss. (Ivić 1962:34).

In this way the dialects of a single language system would be seen to be related in an explicit manner. The Quechua example shows that neither tree diagrams, maps, or wave diagrams are adequate to protraying the linguistic relationships in question. The idea of implicational rules in phonology goes back to Jakobson, and has been greatly extended by Greenberg. Chomsky and Halle's (1968:410) hierarchy of the availability of features is another example of this trend. Not long after DeCamp's (1968) paper was delivered, Kiparsky (1968a) proposed something of the same sort to replace diacritic features like [native]. The trend continues to grow, for Sandra Annear (personal communication) in collaboration with Stanley Legum has found that the fluctuating data of English syntax can be reduced to a similar kind of implicational stability.

In one sense a pandialectal grammar is easier to formulate than a dialectal one. The latter must predict the limits of what the speaker using such a grammar may say. But the pandialectal grammar only predicts the possibilities--its listed rules may be used as given, generalized, or reordered in specific ways. If these latter possibilities are not all realized in given idioms of the language at a particular moment, that does not invalidate the pandialectal grammar per se.

Four possible explanations of the child's interdialectal communicational prowess have been suggested. Most of them

need to be tested. The fourth possibility seems to me to be the most interesting and potentially fruitful explanation, and consequently it is the one I prefer to examine. I am aware that the term language system in the foregoing exposition is a weasel word. Without any constraints on the naturalness of underlying representations and rules, and without some requirement that systems may embrace only rules that can be reduced to some sort of natural-language implicational scale, any two systems are relatable, and the idea under discussion is an empty notion. The only way that I see of avoiding the circularity of defining a system by considerations of naturalness and defining naturalness by what is to be found in linguistic analyses is to begin by accepting the agglomerate of English standard dialects as a system and to go from there. If synchronic rules sometimes look different from diachronic rules, they must have got that way by generalizing diachronic kinds of changes (Kiparsky 1968a). Or the analysis may be wrong in that the unnatural rule should be reduced to two or more natural ones (Postal 1967).

What remains to be shown is that certain differences between diachronic and synchronic analyses do not materially affect the position being taken here. First it is necessary to distinguish language systems from quasisystems. A language system subsumes different idioms under common underlying representations (with deviations limited by certain tolerance thresholds yet to be discovered through empirical investigation) and a single set of rules which can be organized on an

implicational scale to which the Quechua example cited is no doubt only a remote approximation. (I am thinking in terms of a language model containing an ordered set of rules in which the phonological rules are not blocked off from the others in a separate component.) In contrast with this, a quasisystem contains different systems not so neatly reducible to the unity of a language system, but nonetheless relatable through some similarities. One might look for this similarity in a reasonably unified underlying representation (SPE 50, 54). But, wherever it is to be found, it must exist if language systems are to be related in time or space. We should expect to find that Proto-Germanic and the relations between Old and Modern English constitute quasisystems of the sort in question. If different languages do not fall at least into a quasisystem, they cannot be related. The degree to which Proto-Indo-European could be reconstructed using only the evidence of the contemporary languages would be insignificant in comparison with what can be accomplished with the data of the older languages like Sanskrit that form a quasisystem with the mother tongue. Hence, quasisystems are relative entities. But the point of the discussion is that the relatability of linguistic idioms in time and of those in space are alike in theory and method. Perhaps the elusive linguistic difference between pidgins and creoles lies in the supposition that creoles fall into a systematic relationship with the standard language that is not discernible for pidgins.

One difference in the handling of data has to do with relics (e.g. the relationship of English drag, draw, draught). Presumably both synchronic and diachronic analysis would begin by formulating a rule for the relationship. This would be subsequently rejected by the descriptivist as more expensive than lexical allomorphs and on the grounds that it falsely represents a generalization of current English grammar. The historical linguist would react differently, treating the relic as evidence for a formerly productive relationship based on regularly generated phonological alternations. And so with foot : feet, a relationship which is a purely arbitrary morphological one in contemporary English. The comparatist then seeks evidence from related systems and from earlier documents to corroborate his suspicions in such matters. Even though the comparatist abandons the economy metric of the descriptivist in his handling of relics, he presumably uses it for the prior system which he postulates on the basis of the evidence provided by the relic. So it remains true that, even though the diachronic analyst and the synchronic analyst employ the analytic methods for different ends, the methods do not differ as such. The same is generally true of the theory underlying those methods. The theory of internal reconstruction has received many contributions both from the synchronic side (SPE) and from the diachronic side (Kiparsky 1968b). One may look for similar developments with respect to the comparative method, once descriptivists begin to view the task of formulat-

ing a grammar that reflects the hearer-speakers competence, or knowledge of his language, as one that goes beyond describing single idiolects or dialects and to use the comparative method in the formulation of such a pandialectal grammar. In the process of unifying the theory and method (but not the aims and admissible data) of diachronic and synchronic analysis, it will become possible, one may predict, to apply the constraints on descriptive grammars to historical reconstructions and rule out representations and rules in the latter which are known to be inadmissible in the former. This would spell the end of segmental inventories and phonotactic sequences which are obviously unnatural. Comparatists could no longer retreat from reality with the subterfuge that their proto-segments are merely lables for correspondences lacking any claim to phonetic reality.

A more serious possible difference between diachronic and historical analyses is posed by George Grace's (personal communication) suggestion that language changes are formulable as epirules that prescribe changes in synchronic rules. Such epirules might direct deletions of features in (generalizations of) rules, deletions of entire rules, reorderings of rules, and the addition of new rules. They would be governed by constraints to the effect that rule changes could only involve generalizations, for example, and that reorderings must be in the direction of unmarked sequencing. Lexical and morphological epirules would prescribe segmental changes throughout a given portion--sometimes one hundred per cent--of the lexicon.

Such changes would involve respellings of nominalization morphemes, for example, or the resemantification of morphemes whose spelling remained unchanged. Such changes could involve across-the-board changes of given segments in the lexicon by altering one or more features in them. A new denominative adverbializer (<u>-wise</u>) can be created without greatly affecting the over-all system. Syntactic rules can be generalized by epirules prescribing the appropriate changes.

It seems to me that such epirules would not have the status of real rules but only prescribe before-and-after relationships which would be governed by the same naturalness conditions as the rules they create by adding or altering prior rules of the language system. Even relexifications like Yid. <u>vek</u> and <u>op</u> (Kiparsky 1968b:177) can result from the application of epirules solely to items exhibiting morphophonemic alternations in a perfectly natural manner. If nothing else, epirules of the sort under consideration exemplify the relatability of prior and posterior systems. In no respect do they, as such, lend support to the Saussurian theory of the unrelatability of such systems. De Saussure regarded a single change as potentially creating a new system of relationships among the elements of the language and regarded what are being called epirules here as blind, accidental, and capricious, despite his admission (de Saussure 1959:88 = 1962: 126) of a framework of universal conventions within which language change takes place. For de Saussure diachronic rules of the kind under discussion, e.g. $X \rightarrow Y \; / \; ___ \; Z$, were

ruled out because in the new system created by the change to Y the segment undergoing this feature change would become something that could not be compared with anything in the earlier system containing X. But if this were true diachronically, it would also be true synchronically and de Saussure could not maintain the interpersonal definition of langue espoused by him. I am prepared to entertain the view that, if epirules are needed diachronically, they will also be required in synchronic analyses to relate comparably different systems. In either kind of analysis we are dealing with the same human being. It is also the same linguist that may be doing diachronic analysis in the morning and synchronic analysis in the afternoon. To maintain as radical an antinomy between the theory and methods of the two pursuits as has been done would seem to place the burden of proof on the upholder of this inexplicable dichotony, rather than on the upholder of the essential theoretical unity of the two--three, if we add dialectology--linguistic disciplines.

The differences appear much smaller if we do not select different plural formations from different systems (de Saussure's fōt : *fōti and fōt : fēt), and if we do not limit our attention, as he did, to mere surface phenomena. What is meant can be neatly illustrated with a Spanish example from Saporta (1963: 223). The plural of [kláse] is [klásɛs] in Castilian and [klásɛ] in Uruguayan. Superficially, the processes are unrelated. On this level Uruguayan not only has more vowel

phonemes, even if only in word-final position; it also forms the plural by simply changing a vowel, whereas Castilian adds a consonant, the vowel change being merely a predictable and automatic accompaniment of syllable closure. But if we look more deeply into these mutually intelligible idioms we can explain the obvious fact that they belong to the same language system by assuming with Saporta that the only difference between them lies in the Castilian minus value for the Uruguayan rule that deletes word-final //s//.

Another example is discussed by Kiparsky (1968. 199-200). There are Swiss German dialects that both contain a rule umlauting back vowels in the comparative degree of adjectives and a rule that rounds an underlying //ā//, such as is found in the lexical entry of the word for "late"--//Spāt//. The comparative form of this adjective in the dialect which has the two rules in the older, marked order, in which the umlauting rule precedes the rounding rule, is ['špǣtər]. In the dialect exhibiting the unmarked order, in which umlauting follows rounding, the comparative form is ['špœ̄tər], with a tense rounded front vowel that would have to be regarded as a new phoneme in a phonemic analysis. This additional phoneme would, of course, represent a complication of the system relatively to the other system. Generative phonology regards the development as a simplification, since nothing new is added to the underlying representations or rules of the other dialect, and the rule reordering is in the direction of the

universal and presumably innate desideratum. This is of course what one would expect in linguistic change.

If one does not restrict one's attention to purely surface phenomena the two Spanish systems and the two Swiss German systems can easily be united into a single language system by means of the comparative method. But if only surface phenomena are considered, then de Saussure is of course right, and there can be no talk of such unification. Even de Saussure's (1959:97 = 1962:137-38) own example of Cl. Gk. dat. sg. thriksí beside nom. pl. thríkes shows the opposite of what he intended, if his point was to demonstrate that the historical explanation, Grassmann's Law, had been replaced by a mere synchronic morphological irregularity. For the application of the method of internal reconstruction along the lines found in SPE clearly motivates an underlying synchronic representation as //thrikh-// and a phonological rule corresponding to Grassmann's Law.

But the synchronic formulas normally show some loss, as compared with the related diachronic realities. In dialects in which taut, tort, and taught are homophonous, the underlying form of all three could be //tɒ̆rt//. But this internal reconstruction would have to be modified in a pandialectal analysis by the comparative method. Since //ɒ// becomes [ɔ] in various dialects before tautosyllabic liquids, velars (e.g. log, long), and voiceless fricatives other than /š/, it would be logical to try to account for [ɔ] in taut by postulating an underlying

//tɒxt// in a synchronic analysis based on data available to a child unacquainted with the history of his language, since the velar fricative //x// is already independently motivated and since this makes the rule changing //ɒ// to [ɔ] more general. Whether such an //x// historically existed in taut (and tight) or taught is determined in diachronic investigations by documentary evidence unavailable to a child but quite admissible in historical studies.

When we turn from surface phenomena to underlying analyses, we find that historical final schwa is needed in contemporary French, //ï// is needed in Mongolian, //ɵ// is needed in South American Spanish noun plurals; perhaps an umlauting final //i// is needed to generate umlauted noun plurals in German in a natural manner; and vowels systems are needed in contemporary Russian, French, and English which bear remarkable resemblances to their fairly remote ancestors. In view of such considerations, de Saussure's (1959:81 = 1962: 220) claim that "the multiplicity of signs . . . makes it absolutely impossible to study simultaneously relations in time and relations with the system" was unjustified and there is no reason why similar theory and methodology cannot be applied to diachronic and synchronic analysis alike to bring the current schizophrenia in linguistics to an end. What is only a possibility becomes a necessity when we acknowledge the instability of idiolectal data demonstrated by Labov (1966) and the truth of de Saussure's assertion that langue

"is not a function of the speaker" (de Saussure 1959:14 = 1962: 30), whereas parole is and must therefore be excluded from systematic study because "its manifestations are individual and momentary" (de Saussure 1959:19 = 1962:38), and his assertion that langue "is not complete in any one person; it exists perfectly only in the mass" (1962:30, my trans.; cf. 1959:13-14).

(iii)

In order to ascertain the empirical validity of the reasoning set forth in the preceding paragraphs, I have formulated a first approximation to a pan-dialectal phonology of English. It is clear that anything short of a fairly comprehensive analysis would run the risk of offering ad hoc solutions for various small-scale problems that would work quite well in the limited situation, but not in the over-all picture. The set of 110 ordered rules used for the present purpose seems sufficiently large to offer a reasonable test of the hypothesis under consideration. The rules are intended to cover a variety of dialects that I have gained some familiarity with, viz. the dialect of SPE and certain non-standard variants of it known to me; British Received Pronunciation (abbreviated BRP); standard and non-standard New England and Southern States dialects, including Southern Mountain speech; Tidewater Southern States and Carribean English; Cockney speech (Sivertsen 1960); New York City English; Scots; and Hawaiian English. Most of the rules depend on phenomena discovered through personal investigations of the idioms.

An exception to this statement has to do with the accent rules of English, where almost all of what I say is based on the analysis found in SPE.[2] The rules used for this test took those of SPE as their basis and point of departure. This was done because SPE provides the most painstakingly formulated and tested analysis of its type and is the most available one to the general public, and also because this analysis has so few low-level rules that it works for all dialects to a remarkable degree.

The question immediately arises as to what claims even the varied polydialectal assortment used in the present formulation could make to being a pan-dialectal phonology of English. The first observation on this question is that if, as claimed here, the underlying representations and rules of such a synchronic polydialectal grammar are constructed with the help of the comparative method, then one could legitimately expect the same leveling out in the synchronic analysis as is generally found in historical investigations employing this method. After a few not overly leveled-out systems of data have been incorporated into the analysis, additonal data often alter no more than lexical details and generally serve to confirm previous hypotheses or to weight a choice between previously equal hypotheses in favor of one or the other.

It is clear that a dialectal assortment such as the one listed above will be greater in variety than most children would be exposed to, but smaller in quantity. One might question the effects of the order in which a child is exposed

to different idioms on the resulting grammar formulated by him. The leveling out just referred to should cancel great differentiations in the order of acquisition. But this might well not be the case when the data were different in kind. Chomsky has somewhere pointed out that the farm boy who acquired the Romance part of English later than the professor's son might end up with a different grammar. This fear was more reasonable when the theory had no way to get around diacritic features like [native]. But if words of varying degrees of foreignness can be placed on an implicational scale of rule applicability, as recently suggested by Kiparsky (1968a), it seems that the fear just mentioned has less plausibility, at least if the hypothesis that the brain organizes linguistic data in implicational hierarchies is correct. One would like to hear more about this from the psycholinguists. I am willing to concede that the young man who has spent all of his formative years in a socially and linguistically homogeneous community blessed with only a half-dozen age and class dialects that are not widely differentiated will have fewer tools with which to predict new dialect possibilities than the young man who has served in the armed forces and been exposed to many diverse idioms. The latter should presumably know more of the elements of his language and more of the ways in which they get bundled and rebundled than the former. He would consequently bring to a newly encountered idiom a higher previous competence than the young man from the ingrown community. For this reason, the soldier should adapt more rapidly to new dialects.

It is worth considering the alternatives to the notion of leveling just put forward. If one accepts the reality of polydialectal communicational competence, yet denies the leveling out, then it is necessary to conclude that everyone has an essentially different polydialectal grammar from that of every other, or almost every other, speaker-hearer. This would evacuate the value of any one of such polydialectal grammars. The description of a single one would be even less rewarding than the results of the idiolectal analyses now in vogue. On the other hand, the validity of the leveling out would in turn legitimatize the use of historical materials as a discovery procedure for synchronic analysis.

The first approximation to a pan-dialectal phonology of English revealed some interesting phenomena. Before discussing the details, some statistics, admittedly only approximate, in view of the nature of the rule set, may be cited for what they are worth. About sixty-three rules are identical for all the idioms out of the total of 110. A good number more differ only in that some dialects have slightly more generalized forms of the rules than others. About twelve rules are present or absent is a substantial number of dialects, whereas about three are absent in single dialects and fifteen are present only in one or two dialects. Dialectal differences in English mostly depend on differing degrees of the generality of rules essentially common to all or most of them.

But differences of rule ordering also were encountered. Twenty or more of these differentiated dialects; five served

only to create lexical exceptions in a given dialect, the exceptional items having the unmarked ordering; and two ordering differences functioned in both ways. A total of twenty-seven or more such differences, then, were encountered. A rule could skip over as many as twenty-six intervening rules to get into an unmarked ordering. One would like to know from the psycholinguists whether the increase of intelligibility of the unmarked ordering over a marked ordering is less when a rule has to move over a larger number of rules than over a smaller number in order to get into the unmarked position (Larry Martin personal communication).

Although considerations of space forbid illustrating the changes in rule ordering in detail, two matters of some theoretical interest did emerge from the analysis and deserve comment. There is a rule, which I call the second palatalization, that, in this conjunctive order, palatalizes //x// before /y/ and sibilants before any palatal. As the word question ['kʍɛščən] shows, the second palatalization of sibilants has to follow the first palatalization, which palatalizes the alveolars (and therefore not /þ ð r/) before /y/, which is deleted in the same process (cf. culture, pressure). Other examples of the second palatalization of sibilants are horse-shoe, miss you, misuse. The palatalization of //x// must follow the triphthongization of certain vocalic sequences, in order to yield [ç] from /xy/ in Huey ['çuɪ] (cf. Hugh ['hiũ]) and here, BRP ['çɜ:], SS ['çɛə] (cf. ['hɪə 'hɪə]). In dialects having [š] in shrimp and shrub, the palatalization of //x//

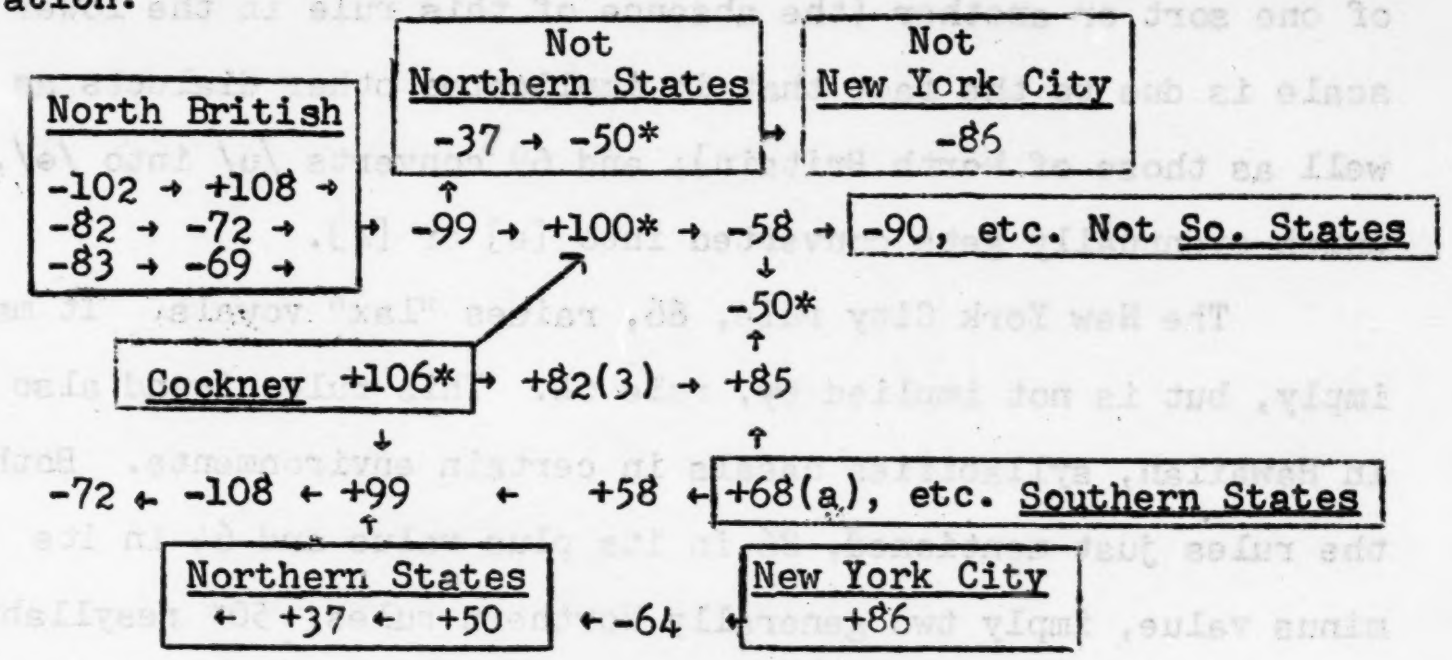
must follow the specification of the palatal character of this sibilant. The analysis undertaken by me showed that the simplest grammar would bracket the palatalization of //x// before /y/ together with the palatalization of sibilants before palatals. The phrase miss Huey shows that the subrules must come in the order mentioned, at least for idioms having a palatal sibilant in this phrase; other idioms would have the reverse ordering. As already stated, the word question shows that the second palatalization of sibilants must follow the first palatalization rule, which, however, has the unmarked order after the triphthongization rule and the palatalization of //x// in dialects that have [č] in mature and right here. If this reordering to a position between bracketed subrules is rejected, then a bracketed subrule must be moved out of its rule and put before the first palatalization rule; at the same time, the triphthongization and other rules would similarly have to be relocated, in order to keep their position before the palatalization of //x// and simply to create the similarity in some idioms between perpetuity and perpetual, where //t// is realized as [č] in both words.

Another point worth commenting on is the truncation rule. This handles cases like Jer' (for Jerry; F. W. Householder personal communication) and Sal (for Sally). When truncation does not take place, the rule that diphthongizes a liquid with a preceding accented vowel must, as is obvious, precede the rule that neutralizes certain vowels before the tautosyllabic

satellite [ɚ]. (Different treatments [Bailey to appear] of intervocalic liquids produce the different pronunciations of Mary, merry, marry; only dialects that change the liquids into satellites which are syllabified with the preceding vowel show the vowel neutralizations.) But the utilization of the option which truncates Jerry to Jer' and Sally to Sal triggers a reordering in which neutralization does not follow the rule that creates liquid satellites in these words in dialects syllabifying the liquids with the second vowels of untruncated Jerry and Sally. The operation of truncation also produces a reordering that prevents /æɫ/ from going to /ɑɫ/ (cf. various pronunciations of calm[ative], alternate, falcon, etc.). In the absence of truncation, some dialects reorder the rules so that the /ɑɫ/ created by the last-mentioned rule then becomes the input to a rule that changes /ɑɫ/ from underlying //ɔl// to /ɔɫ/. The net result of the new ordering is to make doll rhyme with ball.

Turning from these brief illustrations of the reordering phenomena and getting back to dialectal differentiations based on the presence or absence of particular rules or of more general forms of rules, one wishes to know whether these differences can be ordered on an implicational scale along the lines of the one discovered in the Quechua example. It would be expected that the implicational scale for the 110 rules included in the present analysis would be more complex than the one which it was possible to formulate for the more limited Quechua example. But the success of that analysis gives one courage

to attempt a similar formulation for the English data. In the following formulation an asterisk stands for the most general form of a rule that is less general in other dialects. The rules which are similar for all the dialects are left out of consideration.



The numbers which are denoted by the numbers in the above formulation are as follows:

-90, etc., denotes a block of nine rules which is the mirror-image of the block denoted by +68(a), where the values are all plus and the arrows point in the opposite direction. Rule 90 retracts "lax" vowels in certain environments. Rule 68a diphthongizes the nuclei in push and wash.

Rule 106* drops initial /h/ (Cockney psilosis).

The rules in the North British bloc are 102, which differentiates the "voiced" and "voiceless" orders of obstru-

ents on the basis of the duration of the preceding vowel; 108 converts /r/ into a uvular vibrant; 82 is a vowel-shift rule; 72 is the neutralization rule mentioned earlier; 83 is the late diphthongization rule that converts /ē/ and /ō/ into diphthongs of one sort or another (the absence of this rule in the lower scale is due to the fact that it implicates other dialects as well as those of North Britain); and 69 converts /u/ into /ɵ/, which eventually gets converted into [ə] or [ʌ].

The New York City rule, 86, raises "lax" vowels. It may imply, but is not implied by, rule 64. This rule, found also in Hawaiian, syllabifies nasals in certain environments. Both the rules just mentioned, 86 in its plus value and 64 in its minus value, imply two generally Northern rules: 50* resyllabifies intervocalic non-nasal sonorants, and 37 changes unaccented /i/ to /ə/ except before certain boundaries.

The implicational formula shows [-50*] in two places. This shows that it requires some improvement.

The Cockney rule (vide supra), 106*, implies certain vowel shifts--rule 82(3). Both the last and the Southern States idiosyncratic rules imply rule 85, which fronts the vowels of foot and food. This last implies the absence of rule 50*, already mentioned; the absence of 50* is also implied by the absence of 58, the rule that nasalizes vowels (and deletes the nasal) before tautosyllabic "voiceless" consonants.

All the rules in the chart have been mentioned except two. Rule 99 changes //t// to [d] or a flap before unac-

cented vowels under certain further environmental constraints. Rule 100* rounds the palatal strident segments.

The formulation that has been given is by no means so complicated as it seems, in view of the almost mirror-image nature of its upper and lower halves. The upper scale is generally British; the lower one, American. A number of minor rules that depend on whether a dialect has the rule that changes /ɔ/ to /ɑ/ when not before vowels would form a small scale of their own. The following further comments can be made on this implicational formulation.

1) There are dialect blocs of rules with their own subordinate set of implications. Perhaps to hearers outside such dialects any single rule can function in place of the whole bloc. This would greatly simplify the diagram, which, incidentally, omits various substandard rules in the total analysis. For a Britisher who had not heard American English, or for someone on this side of the Atlantic who was unfamiliar with British speech habits, there would be an increase in the number of implications that could be added to the formulation. It should be noted that some dialect types, particularly the Southern States and to a lesser degree Northern British, are characterized by a larger number of idiosyncratic rules. Other dialects have very little that marks them off from the others. This observation will receive further comment below.

2) There are some rules, which happen to be those which are most noticeable to the non-specialist, that neither imply nor get implied by other rules. These are, moreover, generally

late rules. They often exhibit fluctuation within a single dialect region. Thus we find "r-less" and "r-ful" speakers in New York City, in the Southern States, in New England, and in Southern England. We find [əɪ] in <u>wife</u> in such diverse dialect areas as Scotland, Charleston (South Carolina), and Toronto, not to speak of northern Pennsylvania. And /uə/ gets lowered to /oə/ or /oə/ in the Southern States, in Wisconsin, in Great Britain, and in the Tidewater South. The rule that nasalizes vowels (and drops the nasal) before tautosyllabic "voiceless" consonants also exhibits a similar regional diversity. It cannot be considered a point in favor of traditional word geography that it has concentrated on such superficial and unrevealing aspects of English pronunciation in drawing up its dialectal boundaries.

3) The residue of minor implications (e.g. dialects that have [æu] in <u>drove</u> will have [ɛɪ] or a diphthong with an even opener peak in <u>raid</u>) is trivial by anyone's account.

A final comment is in order. The dialects with few rules found only within their own type will probably be more intelligible to dialects have many idiosyncratic rules than vice-versa. On the other hand, they would provide a less significant testing ground for phonological theory than those complex dialects whose analysis would require a much more sophisticated theory.

The question now arises as to whether the implicational formula exhibited above is one which the speaker-hearer's brain

can accommodate; does it go beyond the limits which a single natural-language system tolerates? Since those limits are determined by what language systems evince, the only way to avoid circularity in dealing with this issue is to begin with the assumption that the dialects included in the present analysis do in fact constitute a single (mutually intelligible, in fact) linguistic system and then to accept what this system evinces as an example of what natural languages do in fact tolerate. One assumes that the rules exist in their most generalized formulation and marked ordering in a pandialectal hearer's grammar.

All that the writer can hope to have shown is that the idea of a pandialectal grammar is at least a live option and one possible way to escape the communicational dilemma which the acceptance of the idiolectal approach gets us into. Perhaps some readers will have been convinced of one further thing, viz. that this way of dealing with the realities of communication and the child's polydialectal prowess offers a more interesting line of investigation than the other ways of dealing with such phenomena. The writer hopes soon to be able to deal with intonation, a matter of great importance to dialect identification, in similar terms.

If descriptive linguistics does involve the method of internal reconstruction and a comparative dialectology, the way is open to unifying the various at present isolated branches of linguistic study.

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NOTES

[1]At the Conference at which this paper was read on Feb. 1, I predicted a reordering of the rule deleting the middle vowel of memory and the rule that inserts /b/ in humble (cf. humility) and in tremble (cf. tremulous): memory would become membry in the unmarked ordering. Less than three weeks later I received a copy of Time (1969:70) in which this form, previously unknown to me, actually occurred.

[2]I plan to print a revision of the 110 rules circulated at the Conference in the Working Papers of Linguistics of the Department of Linguistics at the University of Hawaii. Space limitations forbid publishing the rules here. It should be noted that the exigencies of the comparative method demand a treatment of the vowel shift which is quite different from that found in SPE; cf. Bailey (1969).

REFERENCES

BAILEY, Charles-James N. 1969. "The English Great Vowel Shift Past and Present." Working Papers in Linguistics No. 2. Honolulu: Department of Linguistics, University of Hawaii.

_____. To appear. "Dialectal Differences in the Syllabification of Non-Nasal Sonorants in American English." To appear in General Linguistics, Vol. 8.

BECKER, Donald Allen. 1967. "Generative Phonology and Dialect Study: An Investigation of Three Modern German Dialects." Ph.D. Thesis, University of Texas.

CHOMSKY, Noam, and HALLE, Morris. 1968. (Abbreviated SPE.) The Sound Pattern of English. New York: Harper and Row, Publishers.

DeCAMP, David. 1968. "Toward a Generative Analysis of a Post-Creole Continuum." Unpublished paper read at the Conference on Pidginization and Creolization of Languages in Jamaica, April, 1968. To appear in the published volume of the papers, ed. Dell Hymes.

De SAUSSURE, Ferdinand. 1959. Course in General Linguistics. Trans. Wade Baskin. New York: McGraw-Hill Book Company. = 1962. Cours de linguistique générale. Edd. Charles Bally and Albert Sechehaye with the collaboration of Albert Riedlinger. 5th ed. Paris: Payot.

HALLE, Morris. 1959. The Sound Pattern of Russian. 'S-Gravenhage: Mouton and Co.

IVIĆ, Pavle. 1962. "On the Structure of Dialectal Differentiation." Word 18:33-53.

KIPARSKY, Paul. 1968a. "How Abstract is Phonology?" (Duplicated.)

_____. 1968b. "Linguistic Universals and Linguistic Change," in Emmon Bach and Robert T. Harms (edd.), Universals in Linguistic Theory. New York: Holt, Rinehart and Winston, Inc.

LABOV, William. 1966. The Social Stratification of English in New York City. Washington: Center for Applied Linguistics.

_____. 1968. "Contraction, Deletion, and Inherent Variability of the English Copula." Unpublished expanded version of a paper read at the Winter Meeting of the Linguistic Society of America, Chicago, Dec., 1967.

MEILLET, Antoine. 1931. "Caractère secondaire du type thématique indo-européen." BSL 32:194-203.

PARKER, Gary. 1969. "Comparative Quechua Phonology and Grammar I: Classification." Working Papers in Linguistics No. 1. Honolulu: Department of Linguistics, University of Hawaii. Pages 65-87.

POSTAL, Paul M. 1967. "Formal Evaluation and Substantive Linguistic Universals: A Phonological Case." (Duplicated.) Yorktown Heights, N. Y.: International Business Machines.

SAPORTA, Sol. 1965. "Ordered Rules, Dialect Dif'erences, and Historical Processes." Language 41:218-24.

SIVERTSEN, Eva. 1960. Cockney Phonology. (Oslo Studies in English No. 8.) New York: Humanities Press.

STOCKWELL, Robert P. 1959. "Structural Dialectology: A Proposal." American Speech 34:258-68.

Time: The Weekly Newsmagazine. Feb. 21, 1969. "Religion." Pages 70-73.